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RECENT ADVANCES IN SCIENCE, AND THEIR BEARING ON MEDICINE AND SURGERY.

BY

MICHAEL FOSTER.

FROM THE SMITHSONIAN REPORT FOR 1896, PAGES 339–364.

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RECENT ADVANCES IN SCIENCE, AND THEIR BEARING ON MEDICINE AND SURGERY.[1]

By Prof. MICHAEL FOSTER,
Secretary of the Royal Society.

I.

When fifty-four years ago the school of Charing Cross Hospital gathered itself together for its winter work, among the newcomers was a pale-faced, dark-haired, bright-eyed lad, whose ways and works soon told his fellows that he was of no common mold. To-day I am about to attempt the fulfillment of the duty, which the authorities of the school have done me the honor to lay upon me, of delivering the first of the series of lectures which the school has wisely instituted to keep alive, in the minds of those to come, the great services which that lad's strenuous and brilliant life rendered to the healing art. The trust of the Huxley Lectureship provides that the lecturer shall dwell on "recent advances in science, and their bearing on medicine and surgery." I venture to hope that I shall be considered as not really departing from the purpose of the trust if I attempt to make this first lecture a sort of preface to the volume, or rather the volumes, of lectures to come; and since a preface bears a different paging, and is written in a different fashion, from that which it prefaces, I shall be so bold as, with your permission, to make the character of my lecture to-day different from what I suppose will be that of the lectures of my successors. It will, I imagine, be their duty to single out on each occasion some new important advance in science, and show in detail its bearings on the art of medicine. Each succeeding lecturer will, in turn, be limited in the choice of his subject, and so assisted in his task by the choice of his predecessors. I to-day have no such aid. It seems fitting that, for the purposes of this initial lecture, the word "recent" should be so used as to go back as far as the days of Huxley's studentship. If it be so used, I am brought to face advances in science affecting medicine and surgery so numerous and so momentous that any adequate treat-

[1]The Huxley Lecture. Delivered at Charing Cross Medical School, London, on October 5, 1896, by Prof. Michael Foster, Sec. R. S. Printed in Nature, Nos. 1407 and 1408, vol. 54, 1896.

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ment of them as a whole would far exceed not only the time at my disposal, but also, what is more, my powers to treat and your patience to hear. I will not dare so hopeless a task. Nor will I attempt to select what may be deemed, or what may appear to me, the most important of these advances, and expound the bearings on medicine of these alone. I venture to hope I shall best fulfill the duty laid upon me, and meet with your approval, if I single out and dwell on one or two general themes suggested by the history of science during those fifty-odd years.

The first theme is one suggested by a survey of the studies which engaged young Huxley in the school here in 1842. This will bring before us a special bearing on our profession of the advance of science which, though it may not be evident at first sight to everyone, is nevertheless real and important.

Each case of illness is to the doctor in charge a scientific problem, to be solved by scientific methods. This is seen more and more clearly and acknowledged more and more distinctly year by year. Now, it is true that each science has, to a certain extent, its own methods, to be learned only in that science itself; and from time to time we may see how a man eminent in one branch of science goes astray when he puts forward solutions of problems in another branch, to the special methods of which he is a stranger. In nothing is this more true than in an applied science like that of medicine. At the bedside only can the methods of clinical inquiry be really learned; it is only here that a student can gain that kind of mind which leads him straight to the heart of disease, that *genius artis*, without which scientific knowledge, however varied, however accurate, becomes nothing more than a useless burden or a dangerous snare. Yet, it is no less true that the mind which has been already sharpened by the methods of one science takes a keener edge, and that more quickly, when it is put on the whetstone of another science, than does a mind which knows nothing of no science. And more than once inquiry in one science has been quickened by the inroad of a mind coming fresh from the methods of a quite different science. For all sciences are cognate; their methods though different are allied, and certain attitudes of the mind are common to them all. In respect to nothing is this more true than in respect to the methods of medicine. Our profession has been the mother of most of the sciences, and her children are ever coming back to help her. In our art all the sciences seem to converge—physical, chemical, biological methods join hands to form the complete clinical method. This is the real justification for that period of preparatory scientific study which each enactment of the authorities makes longer and harder for the student of medicine. It is this, and not the mere acquirement of facts. The facts, it is true, are needed. Every day the doctor has to lay hold, for professional use, of mechanical, physical, chemical, biological facts. But facts are things which the well-trained mind can pick up and make

use of as it goes along at any time and in any place; whereas the mind which is not well trained will miss the facts or pick up the wrong ones, or put to a wrong use even the right ones which it has in hand.

Now, the ideal training to be got from any science is that of pursuing inquiry within the range of the science according to the methods of the science; in that way only does the spirit of the science fully enter into the man. But such an ideal education is impossible. We are fain to be content in merely making the student know what truths in each science have been gained and how they have been gathered in, such a teaching becoming more and more effective as a training the more fully the student is made to tread in the very steps, and thus to practice the methods, of those who gained the truths.

The more complete the body of any one science the more useful does that science become as a means of training, and hence it is that advance of science has a double bearing on the medical profession. As each science grows, not only does its new knowledge bring to the doctor new facts and new ideas, new keys to open locked problems, and new tools to use day by day, but the incorporated knowledge gains greater and greater power as an instrument to train his mind rightly to use all the facts which come before him.

Let me, in the light of this view, call your attention for a moment to the yoke of compulsory studies under which the young Huxley had to bend his somewhat unruly neck, and compare it with the like yoke which presses, heavily it seems to some, on the neck of the young student of to-day.

I have not been able to find an exact record of the course of studies pursued by Huxley himself at Charing Cross in the years 1842–1845, but I have been privileged to examine the stained and tattered schedule of the College of Surgeons, duly "signed up," for the years 1844–1847, belonging to one who, during some of those years, sat by Huxley's side, who was then and afterwards his friend, and who has won honor for himself and for your school under the name of Joseph Fayrer.

I find that young Fayrer attended during his first year a course of at least 140 lectures with 100 demonstrations on anatomy and physiology, a course of not less than 70 lectures on materia medica, a course of lectures on the practice of surgery, and a course of "The practice of physics," each of not less than 70 lectures, and a course of hospital practice in surgery of not less than nine months. In his second year he again attended the 140-lecture course on anatomy and physiology, and the 70-lecture course on the practice of surgery, and again hospital practice in surgery, taking as well a 70-lecture course in chemistry, a like course in midwifery and hospital practice in medicine. In his third year he once more attended the 140-lecture course in anatomy and physiology, but no other systematic lectures; the rest of his time was devoted to hospital practice. To these demands of the college of surgeons we ought to add, in the case of the ordinary student, the demands

of the company of apothecaries; but the main addition thus caused would be a course of botany.

Such a curriculum differs widely both in nature, extent, and order from that in force at the present day. But I venture to think that if we examine the conditions of the time, we shall find that the authorities of that day were as wise as, possibly wiser than, we of to-day. In judging such matters as these, we and, perhaps, especially they who would drive the student on into learning by the goad of compulsion, must bear in mind that legislative enactments, such as those prescribing a curriculum of study, always exhibit a long latent period; they come into visible existence long after the stimulus which begat them has been applied, long after the need of those things being done which the enactments strive to do has been felt. So long, indeed, is the latent period, that often new needs have arisen calling for yet other regulations before the old ones appointed to meet the old needs have got into working order. Bearing this in mind, we shall find that the course of study prescribed in Huxley's time was wisely chosen to meet the needs of, at least, the time immediately preceding that, if not, indeed, the time itself.

It will be observed that the study of physics, or as it was then more commonly called natural philosophy, finds no place whatever in young Fayrer's schedule, and that the one short course of chemistry, without any practical instruction, which he attended was taken in his second year—in the middle, as it were, of his curriculum, when he was already advanced in his clinical studies.

At the present time the sciences of physics and chemistry have each of them developed into a body of logically coordinate truths, furnishing an instrument of peculiar value for the training of the scientific mind. Moreover the methods of teaching have developed in no less a degree, so that in the laboratory the student follows, at a long distance it is true, but still follows the steps of those who have made the science, and has at least the opportunity of catching something of the spirit of scientific inquiry. In this educational value of these sciences, even more than in the practical utility of a knowledge of the mere facts of the sciences, great as that may be, lies the justification of the authorities when these, desiring to improve the profession by introducing artificial selection into the struggle for existence, insist that all to whom the lives and health of their fellow men are to be intrusted should have learnt at least something of the sciences in question.

In the time of Huxley's studentship both these sciences were in a very different condition. The time, it is true, was one of great awakening. In physics men's minds were busy opening up the hidden powers of electricity; some ten years before Faraday had made an epoch by discovering induced currents; he and others were still rapidly extending our knowledge, one practical outcome of which was the introduction of the telegraph in 1837. But how great has been the onward

sweep in electric science since then; how great the advance in all branches of physics! To realize the great gap which separates the physics of to-day from the physics of then one has only to call to mind that the world had yet to wait some years before Mayer, and Joule, and Helmholtz, and Grove had said their say. In the books which taught young Huxley the laws of physics he found not a word of that great law of the conservation of energy which, like a lamp, now guides the feet of every physical inquirer, whatever be the special path along which he treads.

In chemistry much, too, was being done. That science was in the first flush of success in its attack on the mysteries of organic compounds. Liebig, Dumas, and others were rapidly making discoveries of new organic bodies, and dealing with types and substitution, were beginning to make their way into the secrets of chemical constitution; but then, as indeed for a long time afterwards, progress was taking the form of the accumulation of new facts interesting and eminently useful, but still mere facts, rather than of the gaining of insight into those laws of chemical change of which the facts are but the expression. And the brilliant success of purely organic chemistry was somewhat prejudicing those inquiries in regions where physics and chemistry touch hands, which in these latter days are producing such striking results.

In the days of Huxley's studentship neither of these sciences presented such a body of truths as could be readily used as an engine of mental training, nor had the educational mechanism for thus employing them been developed; a chemical laboratory for the student was as yet hardly known, a physical one wholly unknown. The profession turned to these sciences chiefly for the utility of the facts contained in them. The facts of physics, with the exception of those of mechanism, were but rarely appealed to, and if those of chemistry were in more common use, it was because they threw light on the mysteries of the pharmacopœia, rather than because they helped to solve the problems of the living body. Hence the authority, not without cause, demanded of the student no physics at all, and asked for chemistry only in the midst of his course, when its facts might help him to understand the nature of the drugs which his clinical studies were already bidding him use.

As regards the biological sciences, the time was also one of change, or rather of impending change; the causes of the change were at work, but for the most part were at work below the surface; their effects had not yet become obvious.

In natural history, in what we sometimes now call biology, in botany, zoology, and comparative anatomy, the activity in systematic and descriptive work was great. The sun of the great Cuvier was setting, but that of our own Richard Owen was at its zenith; new animal forms, recent and extinct, were daily being described, the deep was

giving up its treasures, new plants and new beasts, brought home by energetic travelers, were being duly investigated. But this was only a continuation of what had been going on long before.

Of the great biologic revolution which was about to come, there was not so much as even a sign in the skies when Huxley took his seat on the Charing Cross benches, though Charles Darwin was already brooding over the ideas which had come to him in his long voyage.

Two great changes, however, were already beginning—one due to new ideas, the other to improved methods.

The morphological conceptions, of which Von Baer, in his "History of Development," had laid the foundations, destined to make a new science of animal forms, were being carried forward by Johannes Müller in Germany, though, save for the expositions of Carpenter, they had made but little way in this country. Nowhere, indeed, had they progressed far. The man who, perhaps to Huxley himself, was to advance them most, Gegenbaur, was as yet a mere student. Nor, in spite of the beginning made by Von Baer himself, by Allen Thomson, and by Rathke, had embryology made much progress. Kölliker, to whom the science owes so much, had as yet written no line. Still the new ideas were beginning to push.

Of no less importance was the impulse given by the improvements in the microscope. Only ten years before Sharpey, discovering that eminently microscopic mechanism, ciliary action, found that a simple lens was a much more trustworthy tool than the then compound microscope. But in the ten years a great change had taken place, and, during the latter part especially of the decennium, improved instruments yielded a rich harvest of discovery in animal and vegetable life. Prominent among the new additions to truth was increased knowledge of the mammalian ovum, in acquiring which Wharton Jones, Huxley's teacher at Charing Cross, did much. But the most momentous and epoch-making step was the promulgation of the cell theory by Schwann and Schleiden as the decennium drew to its close, and more or less connected with that step was the accurate description by Von Mohl of the structure of the vegetable cell, and his introduction of the word, which, next to the word cell, has perhaps had the most profound influence on the progress of biologic science—I mean the word protoplasm.

Of this wide field of general biologic knowledge the college of surgeons at that time took no heed, or at least made no formal demand. It is true that part of it found its place in the lectures on anatomy and physiology, and in the consequent examinations, but only a small part. It is also true that the lecturer on materia medica had by custom license to roam over almost the whole of nature, and the student in learning the nature and use of drugs took doses of heterogeneous natural history; the mention, for instance, in the Pharmacopœia of castoreum being made the occasion of a long disquisition on the biology of the beaver.

But in this the end in view was the acquisition of facts, not training in scientific conceptions and ways of thought.

The botany, it is true, which unasked for by the College of Surgeons, was insisted upon by the company of apothecaries, though made compulsory on utilitarian grounds as an appendage to and introduction to the Pharmacopœia, did serve the student in an educational way, teaching him how to appreciate likenesses and differences, even small ones, and how to distinguish between real and superficial resemblances. But the time he spent on this was too brief to make it—save in cases where a special enthusiasm stepped in—of any notable effect.

Of the then conditions of that biologic science which comes closest to the profession of physiology, I will venture to say a few words, though I will strive to curb my natural tendency to dwell on it at too great a length.

A great master—Johannes Müller—had a few years before written a great work, The Outlines of Physiology; a work which the wise physiologist consults with profit even to-day, noting with admiration how a clear strong judgment may steer its way through the dangers of the unknown, and the still worse perils of the half-known. A study of that work teaches us the nature and extent of the advanced physiology, which at that day an accomplished teacher like Wharton Jones might put before an eager student like Huxley, and we may infer what the ordinary teacher put before the ordinary student, each perhaps then, as since, eager neither to give nor to take more than the statutory minimum.

When we look into the past of science and trace out the first buddings of what afterwards grow to be umbrageous branches, it sometimes seems as if every time, and almost every year, marked an epoch; it seems as if always some one was finding out something which gathered into greatness as the following years rolled on. But even bearing this caution in mind, the end of the thirties and the beginning of the forties of the present century do seem to mark a real epoch in physiology. All along the line accurate, careful observation, quickened by the rapid growth of the cognate sciences, was taking the first steps to replace by sound views the sterile discussions and scholastic disquisitions which had hitherto formed too large a part of physiological teaching. The first steps had been taken, but the most marked advance was yet to come.

Though the observations of Beaumont had a few years before, by proving that gastric juice was a real thing, and demonstrating its properties, shown the nature of digestion in its true light, the older fermentative and other theories were not yet abandoned by all. Though the conversion of starch into sugar had been recognized, and pepsin had been discovered, the exact action of the digestive juices had yet to be learned; that of pancreatic juice was almost unknown, and bile still reigned as the king of enteric secretions.

In the physiology of respiration the view that the carbonic acid of expired air was formed in the lungs by the oxidation of the carbon of the blood, still found strenuous support; for Johannes Müller found it necessary to argue at great length that the researches of Magnus on the gases of the blood had placed the matter in its true light. It had been suggested that the red corpuscles were in some way also special carriers of oxygen from the lungs to the tissues, but Müller could not regard this as anything more than a mere supposition.

When it is borne in mind that injection with mercury was the one method employed for tracing out the course of the lymphatics, it will be readily understood how imperfect was the then knowledge of the lymphatic system. And when it is also remembered that though Dutrochet had long before used osmosis to help in the interpretation of the movements of liquids in living tissues, the exact researches of Graham had yet to come, it will also be understood why, when questions of absorptions and cognate questions of secretion came under consideration, they were dealt with as questions in such a condition are dealt even nowadays; much was said about them because little was known.

Though Poisseuille, taking up the matter where it had been left by Stephen Hales in the foregoing century, had begun, and the brothers Weber were just continuing, the work of placing our knowledge of the mechanics of the circulation on a sound and exact basis, and though the then teaching of the mechanical working of the heart did not differ widely from that of to-day, the gap which separates the then knowledge of the circulation, even in its mechanical aspects, from that which we possess to-day, is seen in all its width when I remind you that Carl Ludwig's first paper was not published until Huxley had ceased to be a student—until the year 1845. As to all that great part of the physiology of the vascular system which concerns its government by the nervous system, I will only say that in Müller's great work may be read the pages in which he deals with the conflicting opinions and indecisive observations as to whether the brain and spinal cord have any influence over the heart-beat, and in which, marshaling with logical force the arguments for and against the opinion that the blood vessels have muscular fibers in their walls, finally decides that they have not.

In the physiology of the nervous system a momentous advance had been made some few years before, in the early thirties, by the introduction, through Marshall Hall, of the idea of reflex action. This was rapidly supplying the key to many hitherto unsolved physiological and clinical problems. The special functions of the several cranial nerves were being worked out by Majendie, Reid, and others. The former (with Flourens) was also making many experimental researches on cerebral lesions; and, in another line of inquiry, Bidder and Volkmann were preparing the way for discoveries to come by their important studies on the sympathetic system. The physiology of the senses was being vigorously pushed forward by Johannes Müller; but the reader

to-day of Müller's volumes cannot but be struck with the smallness of the space (if we omit all that deals with the senses) which he allots to the nervous system when we compare it with what is demanded in the present day; and no little part of even that limited space is taken up with a consideration of the laws of those "sympathies" which gave to the sympathetic nerves their name, but which have long since dropped out of sight.

Lastly, it must be remembered that many of the speculations of the preceding part of the century had remained barren, and many investigations had gone astray through lack of knowledge of the minuter changes which lie at the bottom of physiological events. Those minuter changes could not but lay hidden so long as there was no adequate knowledge of minute structure. I have already referred to the improvements of the microscope taking place in the thirties, and this soon bore fruit in the rapid growth of that branch of biologic science once called general anatomy, later on microscopic anatomy, and now best known by the name of histology. It is well-nigh impossible to exaggerate the importance of a histological basis for physiological deductions; it is one of the chief means through which progress has been made and must continue to be made. In the earlier days of physiology the grosser features of structure forming the subject-matter of ordinary anatomy guided the observer to the solution of problems about functions; but after a while these became exhausted, having yielded up all they had to yield, and in due time their place was taken by the finer features disclosed by the microscope. These show as yet no signs of exhaustion, and we may look forward in confidence to their standing us in good stead for years to come. We may expect them to last until we pass, insensibly, from that molecular structure which makes itself known by optical changes to that finer molecular structure which is only revealed by and inferred from its effects, which is an outcome of the ultimate properties of matter, and which is the condition, and so the cause, of all the phenomena of life.

The early forties of the present century may be taken as marking the rapid rise of histological inquiry. It is true that, even before this, the labors of Henle had gone far; that in this country the brilliant Bowman had already (in 1840) given to the world his classic work on the structure of striated muscle, and a little later (1842) his hardly less important work on the structure of the kidney; that the sagacious Sharpey had embodied, in Quain's Anatomy, a whole host of important histological observations, and that many others were at work. Nevertheless, one has only to remember how closely the progress of histology is bound up with the name of Kölliker, and to call to mind that Kölliker's first paper was not published until 1841, to see clearly how much of our present knowledge of histology and all that that brings with it, has been gathered in since Wharton Jones taught it to the young Huxley.

If the gap which parts the physiological learning of that time from the learning of to-day is great, still greater is the gap in the teaching. Though at Charing Cross and in some other schools a course of physiology was given, apart from that of anatomy, this was not separately recognized by the College of Surgeons; it demanded simply a course of anatomy and physiology, of which the lion's share fell undoubtedly to anatomy.

In accordance with this, in most schools, at all events the greater part, and perhaps the sounder part of the physiology taught, was that which may be deduced from anatomical premises. Where the teacher went beyond this, he in most instances at least wandered into academical disquisitions and sterile discussions. Only in rare hands, such as those of Wharton Jones and William Sharpey, was the subject so treated as to be of any real use as a mental training for the medical student preparing his mind to view rightly biological problems. The science was not as yet sufficiently advanced to be an educational engine which could be safely intrusted to the ordinary teacher's use. And the method of teaching it, happily recognized now, which alone insures the salutary influences of the knowledge acquired, that of following out in the laboratory the very steps along which the science has trod, was then wholly unknown. It was as a brilliant favorite pupil that young Huxley was encouraged by Wharton Jones to use the microscope himself, and study, among other things, the structures of hairs; he was not led to it, as one of a flock, in a practical course.

Indeed, one kind of knowledge only was at that time demanded of the medical student, in such quantity and in such a way as to render the study of it a real mental training. Not in one year only of his course, but in each year—in his first, his second, and his third year—was the student, who hoped to obtain the diploma of the college, compelled to attend lectures, each course consisting, not as in other subjects of seventy, but of double that number of lectures, on what was styled anatomy and physiology, but was in the main what we now call anatomy. Moreover, the student learned even then his anatomy in the same way that he is bid to learn all other subjects now, not merely by listening to lectures, or even by witnessing formal demonstrations, but by individual labor in the laboratory—in that laboratory which we call a dissecting room. Nowadays it may seem strange to insist that the student should be studying anatomy during all the three years of his curriculum, down to the very end of his studentship. But we must admit the wisdom of it then. At that time human anatomy was the one branch of knowledge which had achieved anything like complete development, and which successive generations of able teachers had shaped into an engine of mental training of the highest value. It was then the mainstay of medical scientific teaching. It was in the dissecting room that the student, of the time of which we are speaking, acquired the mental attitude which prepared him for the bedside. He

there learned to observe, to describe, to be accurate and exact, and the time spent there was wisely judged to be the most precious of his apprenticeship. The shaping of his mind by help of orderly arranged facts was perhaps even of greater value than the mere acquisition of the facts, important as this might be.

The authorities of the time were, I venture to repeat, in my opinion wiser in their generation in making this well-developed, accurately taught science of anatomy the backbone of the medical student's education; they were wise in making relatively little demand on the student in respect to the other sciences cognate and preparatory to medicine, the value to him of which consisted then chiefly in the facts which they embodied; they were also wise in giving him leave to defer his study of them until his knowledge of something of the needs of his future profession should have opened his eyes to the value of those sciences as mere records of facts.

I also, however, venture to think that the advance of these sciences since then has greatly changed their bearing towards the medical student, no less than towards medicine. What was wisdom in the forefathers is not necessarily wisdom in us the children. I have no wish to take advantage of the occasion of this lecture to make an excursion into the troubled land of medical education. But I feel sure—indeed I know—that I am only saying what the man whose name these lectures bear always felt, and indeed often said, when I suggest for consideration the thought that while some choice out of that advancing flood of science which is surging up around us, and all of which has some bearing on the medical profession, some choice as to what must be known by him who aspires to be the instrument of the cure and prevention of disease is rendered necessary by the struggle for existence—a decided and even narrow choice, lest the ordinary mind be drowned in the waters which it is bid to drink. In making that choice, we should remember that an attitude of mind once gained is a possession for ever, far more precious than the facts which are gathered in with toil, and flee away with ease. This should be our guiding principle in demanding of the medical student knowledge other than that of disease itself.

The usefulness, and so the success, of a doctor is largely dependent on many things which belong to the profession viewed as an art, on quickness of sight, promptness of decision, sleight of hand, charm of manner, and the like—things which can not be taught in any school. But these are in vain unless they rest on a sound and wide knowledge of the nature of disease, on a sound and wide grasp of the science of pathology; and this can be taught. By a sound and wide grasp, I mean such a one as will enable him who has it to distinguish, as it were by insight, among the new things which almost every day brings to him that which is a solid gain from that which is a specious fallacy. Such a grasp is only got by such a study as leads the mind beyond the facts into the very spirit of the science.

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But what we call pathology is a branch—a wide and recondite branch, but still a branch of that larger science which we call physiology; it employs the same methods, but applies them to special problems. So much are the two one that it would doubtless be possible to teach pathology to one who knew no physiology; such a one would learn physiology unawares. But at a great waste of time. For physiology, in its narrower sense, being older, has become organized into an engine which can be used for leading the mind quickly and easily into the spirit and methods of true pathological inquiry. The teaching of it as an introduction to pathology is an economy of time. That, I take it, if compulsion be justifiable at all, is the justification of its being a compulsory study.

Further, the methods of physiology, in turn, are the methods of physics and of chemistry, used hand in hand with other methods special to the study of living beings, the general methods of biology. And here again it is an economy of time that the student should learn these methods each in its own science, and this is the justification for making these sciences also compulsory. But in all the regulations which are issued concerning these several ancillary sciences, this surely should be kept in view, that each science should be taught not as a scientific accomplishment of value in itself, but as a stepping stone to professional knowledge, of value because it is the best means of bringing the student on his way to that.

II.

Now let me turn to another theme suggested by what has happened in science and in the profession since the days of Huxley's studentship, and that is the complexity of the bearings of any one discovery, of any one advance, as well on science itself as on the applications of science.

In the garment of science, with which man is wrapping himself round, or rather is being wrapped round, the several threads are woven into an intricate web. As the loom which is weaving that ever-spreading garment takes in new warp and new woof, such threads only of each are taken in as can be fitly joined to those which have come in before; each thread as it is twisted in becomes a hold for other threads to be caught up later on. No single observation, no single experiment stands alone by itself, nor can its worth be rightly judged by itself alone. The mistaken philanthropists who have put restrictions, and would put more on physiological investigations, betray that ignorance of the ways of science, which seems to be a necessary condition of their attitude, when they ask us to state in a sentence the direct application to the good of man of each experiment on a living animal. In the doors of science, each the opening as often of a path as of a chamber, it is not, as such folk seem to think, that each bobbin pulls only one latch. Every experiment, every observation has, besides its immediate result, effects which, in proportion to its value, spread away on all sides

into even distant parts of knowledge. The good of the experiment by itself is soon merged in the general good of scientific inquiry. The science of physiology, and by implication the art of medicine, is built up in part on experiments on living animals; in part only, but that part is so woven into all the rest that any attempt to draw it out would lead to a collapse of the whole.

It is because each experiment or observation is thus a thread caught up in a close-set web, that its value depends not alone on the mere result of the experiment or observation itself, but also, and even more so, on the time at which, and on the circumstances and relations under which it is made. This truth the real worker in science has borne in upon him again and again; it is this which leads him to that humility which has ever been the outward token of the fruitful laborer. He feels that it is not so much himself working for science as science working through him.

Let me attempt to illustrate this by dwelling on some two or three single observations in physiology, made almost at the time or very soon after the time at which Huxley was a student. It will, I think, be seen that each of them has reached a long way in its bearing on the science of physiology and on the art of medicine; that the full effect of each has been dependent both on what went before and on what has happened since, and though they were all made, so to speak, long ago some of their fruits were brought in as it were yesterday, and their full fruition is perhaps not yet accomplished.

I will first invite your attention to a single experiment, for, though repeated on various animals, we may call it a single experiment, which in the fall of the year 1845 Ernest Heinrich Weber, then professor of anatomy at Leipzig, and his brother Eduard Friedrich, reported to an assembly of Italian scientific men in Naples, and of which they subsequently published an account in Müller's Archiv in 1846. Making use of the recently introduced rotating electro-magnetic apparatus (the physical discovery begetting the physiological one), they found that powerful stimulation of the vagus nerves had the unexpected result of stopping the heart from beating.

This single experiment, which I may quote by the way as a typical experiment on a living animal—for it is impossible to imagine how the discovery of this action of the vagus on the heart could have been made otherwise than by an experiment on a living animal—this single experiment has made itself felt far and wide throughout almost the whole of physiology.

In the first place, it has made us understand in a way impossible before the experiment how, through the intervention of the nervous system, the work of the heart is tempered to meet the strain of varying circumstances. As I said a little while back, only a few years before even eminent observers were groping about in a dim light, hotly discussing whether the brain and spinal cord could affect the beat of the

heart. To all these discussions Weber's experiment came as a great light in a dark place.

There is no need for me to insist how this knowledge that impulses descending the vagus slow or restrain the heart beat, and the knowledge genetically dependent on this, that impulses reaching the heart along the cardiac sympathetic nerves from the thoracic spinal cord stir up the heart to more vigorous or frequent beats, have since served as a guide for the physician in the intricate problems of cardiac disease, and that with increasing security as our knowledge of the details of the actions has increased. The knowledge may not always have been wisely used. On this point perhaps I may be allowed to repeat the caution which I may have given elsewhere concerning the dangers of taking a new physiological fact direct and straight, raw and bleeding, as it were, from the laboratory to the bedside. The wise physiologist takes care, even in physiology itself, not to use a new fact as an explanation of old problems without a due testing and a direct verification of its applicability. How much more is it needful that the doctor who sails not on the calm seas of the phenomena of health, but amid the troubled tempests which we call disease, should not hastily and heedlessly rush to make practical use of a new fact, tempting as the use may be, until he also has tested its applicability by that clinical study which is his only sure guide. But this is by the way.

In the second place, as a mere method Weber's discovery has in physiological experimentation borne most important fruit. Before Weber's experiment many an investigation, not only on the vascular system itself, but in many other branches of physiology, came to a standstill or went astray because the experimenter had not the means on the one hand to stop or slacken, or on the other to quicken and stir up the heart without interfering largely with the object of his research. Thanks to Weber's experiment and what has come out of it, that can now be done with ease, and thus solutions have been obtained of problems which otherwise seemed insoluble.

In the third place, the experiment has had a profound and widespread influence by serving to introduce a new idea, that idea which we now denote by the word inhibition. Before the experiment, though men's minds were gradually getting clearer concerning the nature of a nervous impulse, all known instances of the action of a nervous impulse had for the result an expenditure of energy; and it was a still open though hotly debated question whether in such actions as when a muscle was thrown into contraction by a nervous impulse this feature of expenditure was not impressed on the muscle by the very nature of the impulse itself. That question the experiment answered in the negative once and for all. Whatever the exact nature of a nervous impulse, it was evidently of such a kind that it might on occasion check expenditure and bank up energy in an increased potential store. Observation soon showed that the heart and vagus was no solitary example. It was

recognized that the due regulation of many of, if not all, the so-called nervous centers was secured not merely by the intrinsic forces of passive rest making themselves felt in the absence of stimulation, but also, and even more so, by the alternating play of antagonistic influences. Throughout all the sciences the resolving a stability seemingly due to intrinsic causes into an equilibrium arising out of the balance of opposing forces has again and again marked a step forward; and it is perhaps not too much to say that a like analysis, prompted by the story of the vagus and the heart, has profoundly modified all our conceptions of the way in which nervous impulses, sweeping along the intricate yet ordered network of paths in the brain and spinal cord, determine the conduct of life. The idea has of course been abused as well as used, as what idea has not? Such a word as inhibition could not but fail to have a blessed sound in the ears of the ignorant; the idea has been ignorantly and wrongly applied; but this is of little moment in view of the help which it has given to wise and well-directed inquiry.

And the idea has spread with fruitful results beyond the limits of nervous impulses; it has been carried deep down into the very innermost molecular processes of life. The closer we penetrate into the physical-chemical events through which living matter grows, lives, and dies, the clearer does it seem that life itself is a shifting outcome of two opposing sets of changes—one synthetic, constructive, the other destructive, analytic—and that the key to this and that riddle of vital action lies within the grasp of him who can clearly lay hold of the mutual relations of these conflicting changes. The story of the vagus and the heart is a tale, not of the heart alone, not of the nervous system alone, but of all living matter. The light which first shone in the experiment of the brothers Weber may, in a sense, be said to have gone out into all the lands of physiology.

Let me now turn your attention to an experiment made a few years later. This is also an experiment made on a living animal, and whatever good may have come out of that to which it has given rise must be reckoned as the fruit of an experiment.

In 1851 Claude Bernard made known that division of the cervical sympathetic led to a widening of the blood vessels and a warming of the ear and other parts of the head and neck. This was the beginning of what may rightly be called the great vaso-motor knowledge. It may be true that more than a hundred years before, in 1727, Du Petit had observed much the same thing, but nothing came out of it; the germinal time had not yet arrived. It may be true that other observers since Du Petit had divided the cervical sympathetic and noted the effects; but these had their attention directed chiefly to changes in the pupil. It may be true that Brown-Séquard and Waller a few months before Bernard himself was able to do so supplied the complement to the original experiment by showing that stimulation of the peripheral part of the divided sympathetic constricted the blood vessels and reduced the

temperature. All this may be true, but there remains the fact that with Bernard's experiment the new light began; that experiment marks the beginning of our vaso-motor knowledge.

I have already spoken of the prolonged discussions which, just before the date of Huxley's studentship, were taking place touching the question whether or no the blood vessels were muscular and contractile. That question had, meanwhile, been definitely settled by Henle's demonstration, in 1840, that the tissue in the middle coats of arteries really consisted in part of muscular tissue of the kind known henceforward as plain muscular tissue. But for some years no use was made of this discovery in the direction of explaining the intervention of the nervous system in the government of the circulation. That began with Bernard's experiment.

It would, I venture to think, be sheer waste of your time and mine, if I were to attempt to labor the theme of the large share in our total physiological knowledge which is now taken up by the vaso-motor system and all that belongs to it, and of the extent to which the physiology of that system has woven itself into pathological doctrines, and helped medical practice. I would simply ask the lecturer on physiology in what stress he would find himself if he were forbidden in his teaching to say a word which would imply that the caliber of the blood vessels was influenced by the contraction of their walls through nervous influence; or ask the student how often, in an examination of to-day, he would have to sit seeking inspiration by biting his pen, or staring at the roof, if he too, in his answers, could never refer to vaso-motor actions. Whatever part of physiology we touch, be it the work done by a muscle, be it the various kinds of secretive labor, be it that maintenance of bodily temperature which is a condition of bodily activity, be it the keeping of the brain's well-being in the midst of the hydrostatic vicissitudes to which daily life subjects it—in all these, as in many others, we find vaso-motor factors intervening; and, to say nothing of the share taken by these in the great general pathological conditions of inflammation and fever, they also have to be taken account of by the doctor in studying the disordered physiological processes which constitute disease, whatever be the tissue affected by the morbid conditions. Take away from the physiological and pathological doctrines of to-day all that is meant by the word vaso-motor, and those doctrines would be left for the most part, a muddled, unintelligible mass. To so great an extent as that which Bernard's experiment began entered into our modern views.

It was Bernard's good fortune, but deserved good fortune, to announce, almost at the same time, two fundamental discoveries. For I venture to claim for his discovery of the formation of glycogen in the liver, briefly indicated in 1850, more fully expounded in 1851, an importance only second, if second, to that of the experiment with which we have just been dealing.

To judge of its importance we must look at it from more than one point of view.

At the time when Huxley was sitting at the feet of Wharton Jones, the teaching of the schools was largely governed by the view that the animal organism, in contradistinction to the vegetable organism, was essentially destructive in its chemical actions, possessing no power in itself of synthetic construction. It is true that the possible synthesis of organic compounds special to the animal body had long before, in 1828, been shown by Wöhler's artificial formation of urea. It is true also that Huber, in the case of bees, and Liebig, in the case of cows, had already shown that wax and fat must be in part manufactured out of something that was not fat. The conclusions, however, of these observers were at best somewhat distant inferences from statistical data; and, in any case, had not as yet made much way in the direction of general acceptance. But Bernard's experiment was in the form of an ocular demonstration. The glycogen which had been formed in the liver could be extracted, could be seen, handled, and, if need be, tasted, a result adequate to convince even a physiological Thomas. We may claim for Bernard's glycogen discovery, that, as the first realistic proof of the synthetic powers of the animal organism it did much to establish a truth, which succeeding observations have only served to confirm and extend, namely, that the animal, no less than the vegetable organism, possesses synthetic powers, and that the want of prominence of these in the ordinary work of the animal body is to be attributed to economic reasons, and not to absence, or even scantiness of power.

But there is another aspect from which the discovery must be viewed.

At the time of which we are speaking, physiologists were still, as they had been of old, largely under the influence of a somewhat mechanical conception of the body as a collection of organs, each of which had its special use or function, the unity of the body being maintained by the mutual adaptation of the constituent organs. This was further developed into the view that when a use of an organ had been satisfactorily made out, when a function had been made clear, all that remained to be done, in the way of research, was simply to inquire how far and in what ways the performance of that function was influenced by changes in the rest of the body, or by external circumstances. It was acknowledged, for instance, on all hands that the function of the liver was to secrete bile, and physiologists in general were content to look forward for future discoveries which should throw light on the exact nature of the mechanism of the secretion, and on why the liver secreted now more, now less bile, and to these alone without expecting anything else.

Bernard's discovery that the liver not only secreted bile but manufactured glycogen fell on physiologists like a bolt from the blue. The knowledge that the same hepatic cell was engaged both in secreting bile and manufacturing glycogen, and that the sugar or other prod-

ucts of digestion were carried from the intestine, not straight to the tissues which they were destined in any case ultimately to nourish, but to the liver, there to undergo transformation and await some future fate, marked the beginning of a new way of looking at the problems of nutrition. It was recognized that these became less simple—more complex than they had formerly seemed; but the very complexity gave hope of possible solutions. It was seen that as the blood swept in the blood stream through the several tissues it might undergo profound changes without any visible outward token, such as that of the appearance of secretion in the duct of a gland or of the contraction of a muscle—might undergo changes which could only be demonstrated by differences in the composition or properties of the blood as it came to or left this or that tissue. The technical difficulties of the analysis of blood prevented any immediate marked steps in the way of advance, and attempts to establish in respect to any particular tissue the changes which the blood underwent in it, by inference from the results of experimental interference, met with difficulties of another but no less serious kind. Hence the world had to wait some little time before the new idea which Bernard's discovery had started bore important or striking fruit. Yet it was not very long before it was seen that the hepatic cell had heavy duties touching the metabolic changes of proteid as well as a carbohydrate material; that it, and not the kidney alone, had to do with urea as well as sugar, and the difficulties, which physiologists in the early half of this century must have keenly felt—how to reconcile the bald task of secreting bile, which alone technical physiology alloted to the liver, with the overweening importance which not only popular experience, but more exact clinical study, could not but attach to that organ—began to steal away. A little later on exact experimental inquiry converted into certainty the suspicions which clinical study had raised, that the blood in streaming through the thyroid gland underwent changes of supreme importance to the nutrition of the tissues of the body at large. Still, a little later, the Bernardian idea, if I may so venture to call it, doubling, so to speak, on itself, led to the discovery that the mysteries of the fate of sugar in the body were not lodged in the liver alone, but might be traced to the pancreas. It was seen that as the blood streaming through the liver worked on sugar besides secreting bile, so the pancreas, besides secreting its marvellous omnipotent juice, also influenced, though in a different way, the career of sugar in the body; that the disease we call diabetes was or might be in some way connected with the pancreas no less than with the liver. I need not go on to speak of recent researches on the suprarenal capsules or of other organs. It is enough to note that one of the most promising lines of inquiry at the present day is that relating to the changes of which I am speaking, sometimes known under the name of "internal secretion." Every year—nay, almost every month—brings up some new light as to the details of the great

chemical fight which the blood is carrying on in all the tissues of the body. It may be perhaps to-morrow that we shall learn of some work of a kind wholly unexpected which is carried out by that great Malpighian layer of the skin which wraps round our whole frame. In any case, the line of inquiry is one of the most fruitful of those of the present day. I may add, too, I think, that it is one which has been of the greatest direct use to mankind, and promises still more. It is true that Bernard's discovery of glycogen, and perhaps especially the diabetic puncture, raised hopes which have not been fulfilled. Not to-day, any more than forty years ago, is it in our power wholly to remove the disease which we call diabetes. But short of complete mastery, how great is our power now compared with then. And when we remember that the pancreatic relations of sugar are far from being worked out, and that such knowledge as physiologists already possess has not yet made much way in clinical study, we may look forward to marked progress possibly in no very distant time.

Further, if there be any truth in what I have insisted upon—that the value of a discovery is to be measured not only by its immediate application, theoretical and practical, but also by the worth of the idea which it embodies and to which it gives life; and if it be true, as I have suggested, that by the genesis of ideas the discovery of glycogen is mother of all our knowledge of internal secretion, in its widest sense, of the work of the thyroid and other like bodies, then the good to suffering mankind which may be laid to the door of Bernard's initial experiment is great indeed.

The next result to which I will call your attention is again an experiment, and once more an experiment on a living animal. In 1850 Augustus Waller described in the Philosophical Transactions the histological changes which division of the hypoglossal and glossopharyngeal nerves in the frog produced in the fibers of the distal portions of the nerves, and shortly afterwards developed this initial result into the more general view of the dependence of the nutrition of a nerve fiber on its continuity with a cell in the central nervous system, or in the case of afferent fibers, in the ganglion of the posterior root.

This discovery was at the time and has since continued to be of value as a contribution to physiological ideas. It had its share in promoting the progress—which, though slight, is still a progress—of our understanding the obscure influences which the part of a cell inclosing the mysterious nucleus exercises over all the rest of the cell, and perhaps even to-day the theoretical value of that degeneration of nerve fibers the knowledge of which we owe to Waller is not adequately appreciated and the lead which it gives not followed out as it might be. In spite of all we know, we are much too apt to fall back on the conception that when no nervous impulse is traveling along a nerve fiber the nerve fiber is in a state of motionless quiescence, and that a nervous impulse, when it does come, sweeps over the fiber as a

wave sweeps over a placid lake; but the Wallerian degeneration gives such a view the lie direct. When we reflect that the finely balanced molecular condition, which itself is nothing more than the falsely seeming quiescence of an equilibrium of opposing motions in the ultimate fibrils of the nerve twigs in the ultimate phalanx of the finger, by which we touch and get to know the world without us, is dependent on what is going on around the nucleus of a cell or the nuclei of some cells in the ganglion or ganglia of certain upper spinal nerves, so that if the continuity of the axis cylinder process be anywhere broken the figure of the molecular dance changes at once and riot takes the place of order. When we reflect on this it is clear, I say, that between the molecules of the ultimate fibrils branching in the Malpighian layer of the ball of the finger and the molecules within the immediate grasp of the nucleus of the cell from which those fibrils start there must be ever-passing thrills—thrills, it is true, of so gentle a kind that no physical instrument we as yet possess can give us warning of them, so gentle that compared with them the wave which carries what we call a nervous impulse must appear a roaring avalanche, but still thrills the token of continued movement; and of such gentle, impalpable, unnoticed thrills we must in the future take full account if we are ever to sound the real depth of nervous actions.

It is not, however, as a contribution to theoretical conceptions, but rather as a method, that the results of Waller have so far had their chief effect on the progress of physiology and medicine, and I have chosen it as a thing to dwell on because it seems to me a striking instance of the value of a method merely judged as a method, and, further, because the value of its use illustrates my theme, that the success of any one scientific effort is contingent on the converging aid of other efforts. For some time, it is true—for years, in fact—the Wallerian method was employed solely or chiefly in what, without reproach, may be called the smaller problems of physiology. It settled many topographical questions. It cleared our views as to the distribution of afferent and efferent fibers. It seemed to add or replace a few stones here and there in the growing building, but it did not greatly change the whole edifice. After a while, however, it met with two helpmates—the one sooner, the other later—and, by means of the three together we have gained and are still gaining such additions to our knowledge of the ways in which the central nervous system works out the acts which make up our real life as to constitute perhaps the most striking progress in the physiology of our time. A wholly new chapter of nervous physiology has through them been opened up.

The one colleague is to be found in the experiments of Fritz and Hitzig; and of Ferrier, again, experiments on living animals—experiments which, by demonstrating the existence of definite paths for the play of nervous impulses within the central nervous system, opened up paths for the play of new ideas concerning the working of that system.

I say "demonstrating the existence of definite paths," for this, and not the topographical recognition of so many centers of hypothetical nature, is the solid outcome of experiments on local stimulation of the cerebral cortex. Views come and go as to what is happening when the current is flitting to and fro between two electrodes placed on a particular spot of the Rolandic area. The solid ground on which each view strives to establish itself is that the particular spot is joined by definite nervous paths to particular peripheral parts. I say "demonstrating the existence of particular paths," but what would have been the demonstrative value of the experiments of stimulation or of removal by themselves without the anatomical support furnished by the Wallerian method? And I may justly include within the Wallerian method not the mere tracking out the degenerated fiber by the simple means at Waller's own disposal, but such finer, surer search as is afforded by the later help given by the newer development of the staining technique.

They who have the widest experience of experiments on living animals are the first to own that in a region of delicate complexity like that of the central nervous system the interpretation of the results of any experimental interference may be, and generally is, in the absence of aid from other sources, a matter of extremest difficulty, one in which the observer, trusting to the experiment alone, may easily be led astray. I need not labor the question what would have been the value of the mere effects of stimulating or even of removal of parts of the cerebral cortex, and whither would they have led us, had the experimental results not been supported and their interpretation guided by the teachings of the Wallerian method. It is not too much to say that the experiments of Ferrier and his peers, brilliant as they were, might have remained barren, useful only as isolated bits of knowledge, or might even have led us astray, had they not been complemented by anatomical facts. They have not remained barren and they have not led us astray. The Wallerian method picked out from the tangle of nerve fibers making up the white matter of the brain and spinal cord the pyramidal tract running from the Rolandic area to the origins of all the motor roots, even of the lowest, and so, joining hands with the experiment, made it clear that, whatever might be the exact nature of the events taking place in a particular spot of the cortex of that area, that spot was, by the definite paths of particular nerve fibers, put in connection with definite skeletal muscles. The pyramidal tract was further shown to be merely one—an important one, it is true, but still merely one—of a large class. So it is that the experimental results and the Wallerian results, not merely in that Rolandic area where the results of experiment take on the grosser form of readily appreciated interference with movements, but in other regions where other finer, more occult manifestations of nervous and psychical actions have to be dealt with, are, it may be slowly, but yet surely, resolving that which seemed to be a hopeless tangle of interweaving and interlacing nerve fibers

and cells into an orderly arrangement, of which the key is seen to be that each nerve filament is a path of impulses coming from some spot—it may be from near, it may be from afar—where events are taking place, and carrying the issue of those events to some other spot, there to give rise to events having some other issue.

But a third factor was wanting to forward our insight into this orderly arrangement, and especially by again affording an anatomical basis to open the way toward explaining what was the order of events in the spots or centers, as we call them, in which the filaments began or ended, and what was the mechanism of the change of events. This, I venture to think, we may find in the special histological method which, however much its usefulness, has been enhanced by its subsequent development in the hands of Cayal, Kölliker, and others, as well as by the coincident methyl-blue method we owe to Golgi. The final word has not yet been said as to the exact meaning and value of the black silver pictures which that method places before us; but this, at least, may be asserted that by means of them the progress of our knowledge of the histological constitution of the central nervous system has within the last few years made strides of a most remarkable kind. It may be that those pictures are in some of their features misleading, it may be that the terminal arborization. and their lack of continuity with the material of the structures which they grasp, does not afford an adequate explanation of the change in the nature of the nervous impulses which takes place at the relays of which the arborizations seem the token; it may be, indeed it is probable, that we have yet much to learn on these points. But notwithstanding this it must still be said that, by the help of this method, our knowledge of how the fibers run, where they begin and where they end within the brain and spinal cord has advanced, and is advancing in a manner which, to one who looks back to the days when Huxley was studying within these walls, seems little short of marvelous.

Let me once more repeat, the value of this silver method is not an intrinsic one; it has its worth because it fits in with other methods; it is available on account of what is known apart from it. I imagine that if in 1842 Huxley, at Wharton Jones's suggestion, had invented the silver method it would have remained unknown and unused. The time for it had not then come. The full fruition which it has borne and is bearing in our day has come to it because it works hand in hand with the two other methods of which I have spoken—the Wallerian and the experimental methods.

It is these three working together which have brought forth what I may venture to call the wonders which we have seen in our days, and I can not but think that what we have seen is but an earnest of that which is to come. In no branch of physiology is the outlook more promising, even in the immediate future, than in that of the central nervous system. But surely I do it wrong to call it merely a branch

of physiology. It is true that if we judge it by even the advanced knowledge of to-day, it takes up but a small part of the whole teaching of the science; but when we come to know about it that which we are to know, all the rest of physiology will shrink into a mere appendage of it, and the teacher of the future will hurry over all that to which to-day we devote so much of the year's course, in order that he may enter into the real and dominant part.

There is no need for me to expound in detail how the knowledge gained by the three methods of which I have been speaking, in laying bare the secrets of nervous diseases and opening up the way for successful treatment and accurate and trustworthy prognosis, has helped onward the art of medicine. Even the younger among us must be impressed when he compares what we know to-day of the diseases of the nervous system with what we knew, I will not say fifty, but even twenty, nay even ten, years ago. Do not for a moment suppose that I am attempting to maintain that the great clinical progress which has taken place has resulted from the direct, immediate application to the bedside of laboratory work, or that I wish to use this to exalt the physiological horn. I would desire to take a higher and broader standpoint, namely this, that the close relations and mutual interdependence of laboratory physiology and that bedside physiology which we sometimes call pathology, and the necessity of both for the medical art, are nowhere more clearly shown than by the history of our recent advance in a knowledge of the nervous system as a whole. In this, when we strive to follow out the genesis of the new truths, it is almost impossible to trace out that which has come from the laboratory and that from the hospital ward, so closely have the two worked together; an idea started at the bedside has again and again been extended, shaped, or corrected by experimental results and been brought back in increased fruitfulness to the bedside. On the other hand, a new observation which, had it been confined to the laboratory, would have remained barren and without result, has no less often proved in the hands of the physician the key to clinical problems the unlocking of which has in turn opened up new physiological ideas.

And, though the scope of these Huxley lectures is to deal with the relations of the sciences to the medical art, I shall, I trust, be pardoned if I turn aside to point out that this swelling knowledge of how nerve cell and nerve fiber play their parts in bringing about the complex work done by man's nervous system is not narrowed to the relief of those sufferings which come to humanity in the sick room. Mankind suffers much more deeply, much more widely, through misdirected activities of the nervous system, the meddling with which lies outside the immediate calling of the doctor. Yet every doctor, I may say every thoughtful man, can not but recognize that the distinction between a so-called physical and a so-called moral cause is often a shadowy and indistinct one, and that certainly so-called moral results are often the

outcome, more or less direct, of so-called physical events. I venture to say that he who realizes how strong a grip the physiologist and the physician, working hand in hand, are laying on the secret workings of the nervous system, who realizes how, step by step, the two are seeing their way to understand the chain of events issuing in that sheaf of nervous impulses which is the instrument of what we call a voluntary act, must have hopes that that knowledge will ere long give man power over the issue of those impulses, to an extent of which we have at present no idea. Not the mere mending of a broken brain, but the education, development, and guidance of cerebral powers, by the light of a knowledge of cerebral processes, is the office in the—we hope—not far future of the physiology of the times to come.

I might bring before you other illustrations of the theme which I have in hand. I could, I think, show you that the very greatest of all recent advances in our art, that based on our knowledge of the ways and works of minute organisms, has come about because several independent gains of science met, in the fullness of time, and linked themselves together. But my time is spent.

I should be very loth, however, and you, I am sure, would not wish that I should end this first Huxley lecture without some word as to what the great man whose name the lectures bear had to do with the progress on some points of which I have touched. He had an influence, I think a very great one, upon that progress, though his influence, as is natural, bore most on the progress in this country.

The condition and prospects of physiology in Great Britain at the present moment are, I venture to think, save and except the needless bonds which the legislature has placed upon it, better and brighter than they ever have been before. At one time, perhaps, it might have been said that physiology was for the most part being made in Germany; for, in spite of the fact that some of the greatest and most pregnant ideas in physiology have sprung from the English brain, it must be confessed that in the more ordinary researches the output in England has at times not been commensurate with her activities of other kinds. But that cannot be said now. The English physiological work of to-day is, both in quantity and quality, at least equal to that of other nations, having respect to English resources and opportunities. Part of this is probably due to that activity which is the natural response to the stimulus of obstacles. The whip of the antivivisectionists has defeated its own end. But it is also in part due to the influence of Huxley.

That influence was twofold, direct and indirect. I need not remind you that not only when he sat on the benches of Charing Cross Hospital, but all his lifelong afterwards, Huxley was at heart a physiologist. Physiology, the beauty of which Wharton Jones made known to him, was his first love. That morphology, which circumstances led him to espouse, was but a second love; and though his affection for it grew

with long-continued daily communion, and he proved a faithful husband, devoting himself with steadfast energy to her to whom he had been joined, his heart went back again, and especially in the early days, to the love which was not to be his. What he did for morphology may perhaps give us a measure of what he might have done for physiology had his early hopes been realized. As it was, he could show his leanings chiefly by helping those who were following the career denied to himself. Unable to put his own hand to the plow, he was ever ready to help others whom fate had brought to that plow, especially us younger ones, to keep the furrow straight. And if I venture to say that the little which he who is now speaking to you has been able to do is chiefly the result of Huxley's influence and help, it is because that only illustrates what he was doing at many times and in many ways.

His indirect influence was perhaps greater even than his direct.

The man of science, conscious of his own strength, or rather of the strength of that of which he is the instrument, is too often apt to underrate the weight and importance of public opinion, of that which the world at large thinks of his work and ways. Huxley, who had in him the making of a sagacious statesman, never fell into this mistake. Though he felt as keenly as any one the worthlessness of popular judgment upon the value of any one scientific achievement, or as to the right or wrong of any one scientific utterance, he recognized the importance of securing toward science and scientific efforts in general a right attitude of that popular opinion which is, after all, the ultimate appeal in all mundane affairs.

And much of his activity was directed to this end. The time which seemed to some wasted, he looked upon as well spent, when it was used for the purpose of making the people at large understand the worth and reach of science. No part of science did he more constantly and fervently preach to the common folk, than that part which we call physiology. His little work on physiology was written with this view, among others, that by helping to spread a sound knowledge of what physiology was, among the young of all classes, he was preparing the way for a just appreciation among the public of what were the aims of physiology, and how necessary was the due encouragement of it.

And if, as I believe to be the case, physiology stands far higher in public opinion, and if its just ambitions are more clearly appreciated than they were fifty years ago, that is in large measure due to Huxley's words and acts. I have not forgotten that he was one of a commission whose labors issued in the forging of those chains to which I have referred; but knowing something of commissions, and bearing in mind what were the views of men of high influence and position at that time, I tremble to think of what might have been the fate of physiology if a wise hand had not made the best of adverse things.

One aspect of Huxley's relations to science deserves, perhaps, special comment. On nothing did he insist, perhaps, more strongly than on

the conception that great as are the material benefits which accrue from science, greater still is the intellectual and moral good which it brings to man; and part of his zeal for physiology was based on the conviction that great as is the help which, as the basis of the knowledge of disease, and its applications to the healing art, it offers to suffering humanity in its pains and ills, still greater is the promise which it gives of clearing up the dark problems of human nature, and laying down rules for human conduct. No token, in these present days, is more striking or more mournful than that note of pessimism which is sounded by so many men of letters, in our own land, no less than in others, who, knowing nothing of, take no heed of the ways and aims of science. Cast adrift from old moorings, such men toss about in darkness on the waves of despair. There was no such note from Huxley. He had marked the limits of human knowledge, and had been led to doubt things about which other men are sure, but he never doubted in the worth and growing power of science, and, with a justified optimism, looked forward with confident hope to its being man's help and guide in the days to come.

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